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ABSTRACT

A rationale for being skeptical of drug treatment for hyperactive children is that there is no learning of desired behaviors. Changes in the child's behavior do not carry over to the time when treatment is stopped. The proper approach is viewed as working with parents and teachers to develop behavioral and social means in the problem management of a hyperactive child before resorting to drugs. The author believes that the success of behavioral management shows that the home and school environment can often be changed so that the hyperactive child becomes a happy hyperactive rather than an unhappy, problem child. (SJL)

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WHAT ARE THE PROBLEMS OF A HYPERACTIVE CHILD?

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I would like to begin with a little history. The early 1960's were an exciting time: psychiatry had rested on its' psychoanalytic foundations for a long time and these were being shaken by people who believed that biology could make important contributions to the field. In child psychiatry the concept of "minimal brain dysfunction" brought fresh thinking to a discipline that had accepted psychodynamics as the explanation for virtually all psychological problems that children experience. Knobloch and Pasamanick's work relating "the continuum of reproductive casualty" to later neurologic and behavioral problems of children, and reports by several clinicians that soft neurologic signs were to be found in many patients attending psychiatry clinics, were the main foundations of this new concept.

I began working in the psychiatry clinic of a large university children's hospital at the beginning of this era and I was much taken with the idea of minimal brain dysfunction. I believed that stimulants were the answer to the problems and that the behavior and learning problems that I saw in such numbers were about 90% biologically determined.

I worked in this clinic for nine years so that I was able to follow some of my patients for a long time. My first doubts about the biologic nature of hyperactivity and other problems came when some residents and I did a follow up study of 80 hyperactive children who had attended the clinic for several years. We were not surprised to find that these children continued to have their original traits because we had already realized that it was a

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myth that children grew out of being hyperactive. However I was shocked at the sense that I had not done much for the patients and their parents. Most of the parents had taken their children off drugs (they had all been on stimulants at one time or another) at the time they went to junior high school, and it was hard to believe that the three or four years of drug treatment had made much difference to these patients. Generally their activity level, short attention span, and irritability had improved but the discipline problems and poor school work were worse if anything. I had little stomach for keeping these children on drugs into their teens so I began making more effort with parents of new patients to find ways for them to be more effective in managing their children. To my chagrin though, not many parents tried seriously to change their own behavior and attitudes as long as the child's most distressing problem, his poor showing in school, was being taken care of by drugs. It was like trying to help an anxious adult be more assertive when one was relieving his anxiety with a sedative; the patient does not have enough motive left to over come the normal inertia over changing habits. During the next few years I became convinced that the proper approach is to start by working with parents and teachers to develop behavioral and social means to manage the problems of a hyperactive child before resorting to drugs. In my experience one can nearly always deal with this problems successfully by mounting a combined program for "training" parents and teachers in the skills they need to cope with a difficult child.

What more scientific reasons are there for being skeptical about drug treatment and for preferring a more conservative approach? While there are many short term studies that show the effectiveness of stimulants in making the cognitive sytle of hyperactive children more like that of average children, and which also suggest, though less convincingly, that the social behavior of hyperactive children improves, there is a dearth of information on the results of long term treatment. Recent papers by Herbert Rie and Gabrielle Weiss are about all we have. Rie has found no difference in levels of school achievement between two groups of hyperactive children treated with Ritalin or placebo over periods of six months. Gabrielle Weiss found no difference in social adjustment and the level of problem behavior between three groups of children who had been followed for five years; one group on Ritalin, one on Thorazine, and one not treated

with any drug. The only study of long [illegible] behavioral treatment I know is that of Gerald Patterson. After a two months' parent training period the frequency of problem behavior in a group of children with severe conduct disorders, many of whom seem to have been hyperactive, was significantly decreased, and this improvement was maintained over the next 12 months without further treatment.

It would be surprising if children learnt good habits from taking drugs. The available data on drug treatment for hyperactivity look very much like the more extensive data on the treatment of bedwetting. It has been well established that if you want to cure a persistent bedwetter, that is one who does not respond to encouragement and advice, you should use the alarm system. It may take months but you can ultimately get about nine out of ten bedwetting children permanently dry. The anti-depressants that are used so widely in this country will cut the frequency of wet nights but this effect does not carry over to the time when you stop the drug. In other words there is no learning as a result of drug treatment.

Unfortunately we do not have conclusive data to settle the argument over the relative effectiveness of drug treatment and other approaches, but the data that we have so far do not seem to support the usual arguments for starting drug treatment; that the child will learn to be good while on drugs, that he will achieve his potential in school, and that he will develop better self-esteem.

There is no hard evidence that long term drug treatment hurts children except that it reduces growth somewhat. There is no evidence on whether it sets children up to abuse drugs later, but I expect that we would have noticed a major increase in abuse due to this treatment by now. It may make it harder for adults to counsel against drug use that in every school there are a number of children taking prescribed uppers, but perhaps it does not add much to our hypocrisy since we are already saddled with adult overuse of valium, librium, and other psychotropic drugs.

What I wonder about is the drug's message to a child that he is sick and cannot help his behavior, that he can only be like others when he takes a pill, that not having taken a pill is sufficient excuse for misbehavior such as having a tantrum or setting a fire. This attitude, though compassionate, may damage a child's belief in himself as much as the old moralistic "he is just a spoiled brat". Parents and teachers lose

responsibility too. Believing the problem lies in the child's brain and that it is the physician's job to find a drug that will restore normal function, they give up part of their responsibility and come to depend on the physician and his pills. This may not be healthy for the large number of hyperactive and learning disability children (possibly as many as 5 million), many of whom already suffer from lack of self-respect and whose parents and teachers feel they have failed their children.

Drug treatment seems to me to be basically pessimistic, because there is an underlying assumption that the child cannot control his behavior, nor can parents or teachers, so that the behavior must be controlled by technical means. A behavioral approach, on the other hand, assumes that you do not know what a child can do until you have tried him, and that there are no absolute deficits in a child's self-control. If he can pay attention for one minute, or work quietly with just one other child, you start your training program at that level. Furthermore the child is an active participant in a behavioral program or it is unlikely to succeed, whereas he is a passive object of drug treatment.

Turning from the subject of treatment to the question of diagnosis and the nature of hyperactivity. It remains to be proved that there really is a behavioral syndrome of unusual overactivity, distractibility and difficulty in staying on task, impulsiveness, and excitability. English investigators (Rutter and his colleagues) have impressive data showing that these symptoms occur across all diagnostic categories of children. I have assumed for a long time that these things clustered together more often than by chance, but I am not so sure and I wish that someone would find out. Whether or not it exists outside our offices I am sure that physicians can all too easily be mistaken in diagnosing it, because they tend to make the diagnosis solely on the basis of the parents' account of the child's behavior. School psychologists often see problem children in their classrooms, clinical psychologists may get to see the child's difficulties at firsthand during the testing session, but the physician sees the child in an artificial situation and has no idea what he is like ordinarily. The mother's reports of what is going wrong at home and her view of the teacher's concerns, are to some extent functions of her confidence in herself as a parent, her knowledge and acceptance of the range of normal behavior for boys, the help she gets

from her husband, whether she has outside intererts that are more important to her than her children, and mundane factors like the amount of room there is in the home and whether her husband works nights and needs the children to be quiet so he can sleep during the day. Corresponding factors determine whether a teacher enjoys, accepts, or is bothered by vigorous and assertive little boys. Small wonder that when you are trying to find subjects for research on hyperactive children and you make the mistake of sending people to observe children's behavior in their natural settings, you find that a lot of patients who have classic histories of hyperactivity are ordinary children whose parents have unreasonable expectations, or undisciplined children whose parents do not take the time or trouble to work with them. Other parents may be worn out by marital conflict, or even have some strange investment in the child being diagnosed as a problem.

Another failing in physicians' diagnostic habits is relying on the response to stimulants. Too many physicians assume that if a child responds to a trial of stimulant drugs this demonstrates that he truly is hyperactive. Of course this reasoning is circular, but it naturally follows the doctrine of the "paradoxical response". The burden of proof is on those who assert that hyperactive children respond differently to stimulants from other children. A number of us believe that the specific effects of stimulants on attention to tasks and on motivation are the same as the known effects on normal adults.

Another lesson that I have had to learn personally since I started work on an in-patient unit for the first time is that even children who are so active and aggressive in their homes that deputations of people bring them for admission will behave themselves on a well structured ward. They have to be taught to follow directions but they tend to get hyperactive only when there is nothing to do, or if they are playing with other children, or if the structure is in abeyance for some reason. This experience has taught me that hyperactive behavior which I previously believed was primarily a function of the child's physiology depends a great deal on the prevailing environment.

Our ideas about treatment derive to some degree from what we think are the origins of a problem. If hyperactivity is the result of difficulties at birth it would be hard to argue that it was anything but a defect to be

corrected, whereas if it is primarily a hereditary phenomenon one could argue that it may have evolutionary significance and need not necessarily be treated. Knobloch and Pasamanick made a case for difficulties at birth being related to neurologic and behavioral problems in children, but the large prospective study of perinatal stress by Werner and others of Berkeley University School of Public Health, which involved examining the subjects instead of relating chart data, and which properly controlled social variables, has shown that perinatal stress is not related to behavioral or cognitive problems in 10 year olds, except for lower IQ. In fact this beautifully executed study has underlined the power of social factors.

There are reasons to be equally skeptical about the relevance of soft neurologic signs. Again there are data on both sides of the fence, but the data from controlled studies seem to be on the negative side. For example, in 1962 Prechtl reported a significant association of choreiform movements with reading disability and behavior problems, and other investigators have confirmed this observation. Unfortunately these clinicians did not do their investigations blind, nor did they check the reliability with which choreiform movements were counted. Rutter and Graham found that choreiform movements could only be reliably observed when children had rested beforehand and when the movements were present to a marked degree. Counting only the prevalence of such reliably observed movements, and doing so without knowledge of whether the children had behavior problems or not; these two investigators did not find choreiform movements to be associated with learning or behavior problems in children. Similar comments can be made on the failure of visuo-motor handicaps to predict reading problems.

Finally a biological factor that probably is significant in the origin of behavior and learning problems, namely heredity. There are studies showing that hyperactivity is determined in some cases by genes, probably many interacting genes rather than single pairs as in dominant or recessive mechanisms. If this is true then there are almost certainly biochemical and physiologic differences between hyperactive people and the rest of us. But having such a genotype does not automatically make one a problem. Presumably the expression of this hypothetical genotype depends a great deal on the child's environment. This probably means the earliest interactions between an affected infant and his mother as well as

later interactions between the child and his parents. Let us assume that hyperactive children are often fussy, restless babies who demand a lot of attention; for those of you who know the prospective study of temperament and its relation to later behavior problems by Thomas, Chess, and Birch, this would be their "difficult child". These researchers found that some mothers will cope with a difficult child, but others get anxious, lose confidence, begin to distrust themselves, and to dislike their child. Similarly Howard Moss showed that mothers responded less often to the demands of such babies after a few months. It is not hard to imagine how this beginning leads to a child's learning to misbehave more in order to get attention and to punish his parents for their criticism and dislike. A vicious spiral has started in which layers of learnt misbehavior develop over the original temperament. In other words the problem is not inherent in the genotype, the enzyme defect, the levels of neurotransmitters, or the over- or underaroused state of the brain. The problem arises from the inability of some parents and teachers to manage the child's genotypic personality. These are the people who need our help.

In order to establish that the genotype is inherently a problem one would have to identify all the children in a large population who had this temperament, and show that in most or all cases these children had significant problems and they continued to be seriously handicapped once they had left school and gone into adult life. I do not believe that data from such a study would support the idea that to be hyperactive is to be sick because it seems to me that a number of parents cope successfully with their hyperactive children and so do teachers. I say this partly out of my ordinary social experience, but also out of experience with studies in which we have investigated a fair number of "normal" children for controls. Finally I believe that the success of behavioral management shows that the home and school environment can often be changed so that the hyperactive child becomes a happy hyperactive rather than an obnoxious, unhappy, and thoroughly problematic child.